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Dear Joshua,

Many thanks for sending me a copy of your report. I found this most interesting reading. I am enclosing two copies of my CSH paper. If you do'nt mind,I wo'nt write you now about what I have been doing since I start back to London in three days and have lots of other things to do. Perhaps I may write you on the boat if I am not sea-sick! I would love to have seen you both again for a rather longer period but I fear this is out of the question. One of the frustrating things about my visit to this country has been that I did not have a longer time to discuss things with you at leisure. Perhaps the opportunity will arise in the not too distant future.

I have been working very hard,but rather futilely,here, but have learnt a lot and can now even understand the Rothfels paper! I have embarked,however,upon two interesting lines but in neither case have these yet been followed to completion.

1. Using my Hfr strain,I have analysed the unselected products of zygote segregation in a somewhat similar manner to yourself. I have grown Hfr & F- together in broth for one hour,plated out dilutions on a number of plates of nutrient agar and then spotted colonies containing recombinants by replica plating. I then cut out these colonies,grew them in broth for a few generations,plated again and scored for various markers by replica plating. Unlike you,however, I suppressed growth of the Hfr parent with S so that I do not yet have any information on whether my Hfr parental type appears among the zygote progeny. I did this in my initial pilot experiments because,since I used excess Hfr cells in the initial part of the experiments,it greatly increased the number of F- cells(among which I expected to find a small proportion "fertilised") which I could examine. In my Hfr X TLB_1-F- cross,the Hfr marker S^s is completely absent from prototrophs(500 now tested) so that I assumed the presence of S would not have any effect on the freedom of segregation. Now that I have worked up the technique,however,the first thing I intend to do on my return home is to repeat these experiments without S with the idea of looking for the Hfr parental type among zygote progeny. With my Hfr strain I do not expect to find it. I have examined 18 well isolated colonies which segregated a prototroph and in each have found only one recombinant type together with the F- parental - no reciprocal segregant,although this appears as the sole recombinant type in other colonies.

2. I have begun experiments into the kinetics of recombination using two parents having reciprocal sensitivity & resistance to each of two T phages,thus: Hfr $T_1{}^rT_3{}^s$ X F- $T_1{}^sT_3{}^r$. By adding T_3 at intervals to a mixture of the parentals in broth,the Hfr parent is very rapidly prevented from participating in recombination,so that the time at which recombination is first found after adding T_3 gives the approximate time of zygote formation. In broth,this occurs first in less than 15 minutes while the ratio "zygotes:F- cells" is maximal at about 30 minutes. If the "preformed zygotes" produced in this way are treated with T_1 they lyse,as do the F-. If the zygotes

are spread in replicate on plates of MA + B_1 and treated at intervals with T_1 no prototrophs appear until T_1^r prototrophs have segregated and become phenotypically expressed. This occurs between 4 and 5 hours. If a "Newcombe type" experiment is done on a duplicate series, by spreading with sterile water at the same time intervals, this gives the time at which segregants undergo their first division. This time coincides with the time of phenotypic expression.

I seem to have gone on for longer than I had intended and must stop now. Incidentally, I now think a fusion mechanism is more probable than extracellular transfer - largely because recombination is stopped abruptly when T_3 is adsorbed on to the Hfr parent in buffer- but I still believe that F acts as a vector. And I forgot to add that zygotes are not formed in saline in the kinetics experiment described above.

With every good wish to you both -

Bill